Interview with

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INTERVIEWER: Joshua Lederberg, of the Rockefeller University.

I thought it might be well if we started, just as a general, contextual kind of question, if I might ask you what you consider to be your most important present duty here at the Rockefeller University, as President.

MR. LEDERBERG: Well, I think to identify the direction of the institution, to make proposals that will establish appropriate degree of consensus among the faculty, trustees and the other constituencies, that we have to concern ourselves about.

Q Proposals to what end?

A About the direction, the kind of institution that we are and where we're heading, what our structures and facilities and staffing and identity have to be, and then to find the means of implementing those ideas and keep ourselves oriented appropriately to a set of objectives. That's much easier with this institution than with most others that I've been connected with, since essentially that's our self-identification now, a rather specialized bio-medical research institution as a primary goal. That's going back to its historical tradition, the Rockefeller Institute for Medical Research, and then discovering a limited set of additional functions that are

economically compelling. You have to say you can't afford not to do them because you've already invested so heavily and so effectively in the main floor, that there are few add-ons that can be done at very little incremental cost, and (inaudible) institution and society that we're in. Our educational program has to be described that way. We have a capacity faculty of 200, and 100 graduate students. It's perfectly obvious that graduate education is not driving the institution but we can hardly afford not to do it, given the other resources that we have here.

Q Is this emphasis on the objectives -- You see, I was wondering -- Does this concern for the objectives of the mission of the university and your specifying is to be your prime present duty, reflect a change or a challenge or --

A It does, in some measure, although the groundwork for this has been in place for some years. We'd have to go back into the history of the institution in some ways, and I've described that in my annual report. Between 1901 and about 1950 or '55, there was no ambiguity about the nature of the institution. It was the Institute for Medical Research. At that time, there was a substantial self-examination undertaken by the Board. A committee was established, that was headed by Beth "Bark," who at that time was President of

Johns Hopkins University. There was even a serious consideration, I'm told, of winding up the Institute, with the view that it had been a prototype of the development of a medical research establishment in this country, and had the success of a very substantial degree of imitation around the country.

Well, I'm not sure how seriously to take the view that that was seriously contemplated, but it was not discarded from consideration. It was certainly a very appropriate question to ask, why continue it.

And at that time, Dr. "Bronf" made a number of recommendations which were fitting for that moment and context, so it could be argued, to greatly broaden the scope of the institution, and to transform it into a graduate university which would still be centered on a scientific mission, but was expected to evolve into an operational version of All Souls' College of Oxford, or some of the German graduate universities, and would embrace not only the bio-medical research specialties, but all of the sciences and, indeed, a quite universal scope, including the social sciences and humanities.

This was thought about at a time when there was an enormous wave of expansion and expansionism in American higher education and research. It was the hope for optimism and enthusiasm and funding that was thereafter captured by

the Sputnick reaction in terms of the needs for investment in education and technological development, and may have seemed like very close to our objective, in that context. "Bronf" was sufficiently captured by this concept, that he eventually agreed to leave his post at Johns Hopkins to become the President of the Institute, in order to fulfill this dream. And he did accept the Institution, in motion in that direction, established very substantial expansion of its facilities, pursued a much more aggressive policy with respect to investment in physical plant, opened up applications for substantial funding from federal government sources. Up to that time, the University lived entirely on the income from its endowment. And in fact, sent the institution on its way, with a change in name and style from Institute to University.

This process went a certain length. There was about a three-fold increase in the scope of the institution over a period of 10 or 15 years. Following on that initiative, for the most part, it did not broaden to the extent contemplated. I don't know to what extent there was a formal restatement of goals, but only a limited number of groups were established outside the parameter by medical sciences, but a number were. There were programs in philosophy, intended to be the nucleus of a humanities division. The social -- or I should say, the

behavioral sciences, as it was interpreted here, were started off in a very fine way, starting with the appointment of Carl "Pathman," and included a premier group of people in experimental psychology and some of the dimensions of behavioral science, a program in high "inichi" physics and a program in mathematics and logic were also installed at that time.

Before this process could go much further, but after the institute had become a graduate university, with the initiation of a program of graduate education, pre-PhD educational program, it was more or less arrested by financial constraints, with the changing mood as far as funding is science is concerned, and so on.

I am sure that starting with the early 60's, there began to be a substantial re-examination of those original goals. Dr. Bronf retired, I believe, in 1967, and was succeeded by Fred Seitz, who like Bronf, had been President of the National Academy of Sciences, and had close connection from that sphere. His ecumenism from an intellectual standpoint of the institution, is reflected in the fact, that while Bronf was a bio-physicist and his predecessors had been Flexner, in the first instance, who is a clinical scientist, and Gasser, who is a physiologist of some note, Seitz was not

a biologist at all. He is a solid state physicist, with considerable reputation in that particular field, and at that time, his appointment was entirely appropriate, that the leadership should go to someone who did not fit into what had been the mainline disciplinary specialties at the university. He had a set of skills in terms of overall direction and management that were very well exhibited and verified before he came here.

Almost as soon as Seitz arrived, the university entered into a period of severe financial stringency, and almost his first task was retrenchment, and that was certainly associated with a doctrinal re-examination of what the university was about, since a number of decisions had to be made very shortly, in terms of what programs needed to be sustained.

And there was a gradual reversion during Fred's administration towards the view that the historical strengths of the university had been in bio-medical sciences. That was one reason we had to retreat back to that former mission, in order to create a function in a way that was distinctive, that brought out the distinctive strength and tradition of the institution.

And so it has gone in that fashion throughout

his administration, and the only basis on which I was attracted to coming here was a solidification of that doctine and everyone agreed with that. When I was just approached about the possibility of being interested in coming here, I wrote a doctrinal document about what I thought an organization like the Rockefeller ought to be, not knowing that there had been all of this going on. I thought that would probably resolve any problems or conflicts I might have, and I would not be invited to come. I was quite strenuous with respect to going back to the tradition, and I then discovered that the trustees had been several years ahead of me in this respect, and so, indeed, had the faculty, and there was very strong consensus that exactly coincided with my own views.

So that, I think, may answer your question.

Q It does, indeed.

A There was a piece in SCIENCE about four years ago, coincident with two major events. One was the dropping of the activities in "Bosthee" and the other the sale of some of the real estate the university used to own across the street, which was necessary for cash flow reasons, which I think quite accurately summarizes the pains of the examination I just described.

Some time, someone will write a more detailed

history of who thought what, at what time, and responded to what kinds of influences. There's a great deal of this that I don't know in precise detail, particularly when it comes back to the issues of particular individuals. I'm on very soft ground who the actors were in this respect.

Q You started to comment, when we first got together here this afternoon about the organization of the university, and how the approach the university has taken when compared with the approach that the typical college or university takes, there were some significant differences. I wonder if you would highlight, speak more on this.

A Well, some of those were enjoined by the simplicity of our past, which initially specialized that there's a great deal of community of interest and comprehension of the subject and material of research, which is just not the case at a major university. They have so many disciplines that are so diverse from one another, that you really don't expect a student of medieval French literature to be able to discourse on the latest developments in molecular biology or vice versa, I mean, participate in the critical discussion on either side.

So we have the advantage of possibility in terms of that capability of (inaudible) communication, of avoiding a highly departmentalized structure. We have done

so and that's been a major principle from the very beginning, of the Institute and then the University. The question has come up from time to time about what intra-structure should be established, and it's been rather carefully avoided.

So we have, in fact, 60 laboratories. In our brochure, they're listed -- I don't know if you have a copy.

Q I have it.

A And this is about the only organization that the Institute has. The labs report directly to me.

Q All 60?

A That's correct. There's no one in between. We don't have a dean, we don't have a department head. So we do not have a parametal structure. And while the institution is a little bit large to be able to handle that comfortably, it still works remarkably well. It works as well as any other structure you run into, in many respects much better. It could be compared -- we're about the same size all but the very largest medical schools, for example. We have an annual budget of a little over 40 million dollars a year. In fact, there are 200 all together. We are about the same size as Stamford Medical School or Cornell Medical College. Just a small fraction of a Columbia University or a Cornell University,

but whereas, at Stamford, we have a clinical division and a basic sciences division, and then within those divisions, we have separate departments of medicine and surgery, and OB-GYN, on the one hand, and pharmacology and bio-chemistry and genetics on the other hand, and then individual professors within the departments.

We have none of that in the structure. And one consequence of that is that I do all my work in direct communication with the heads of labs, with the professors who are the main sources of initiative. They are free autonomists and directing the programs within their laboratories, and furthermore, that they have no structures separating them from one another. They're both important, I would say, in talking to one another.

And so to give a fairly trite example, we have a Friday afternoon colloquium, which is an all-university affair, in which we have speakers, two times out of three are from inside the institution. Occasionally, we have guests from outside. And they speak to the entire institutional audience at a quite technical level, and it's a great success. It's one of the outstanding traditions of this place, and I have to be a little bit amused, but one of the questions that was asked of me when I was considering coming, and then was

urged on me again after I had gotten here, was please, could I make a real point of trying to attend those meetings. And of course, I had done nothing else. I have tried, on two occasions, at an interval of a decade, to try to organize that kind of presentation and forum for communication at Stamford Medical School, and it bombed out both times. It was just not the same kind of interest in finding out what was going on in the other departments.

They specialize not only in content, I don't think, really much more than here, perhaps a little, but the whole life of a department revolves around the activities of the department, as a whole, a different set of loyalties and reference, and what you feel you ought to be interested in, and s o forth.

Q How do you relate with these heads of labs, these relatively autonomous heads of labs? I assume they come to you for funding.

A Yes. We have an annual budget cycle and --

Q For each lab? Each lab has its own budget?

A That's correct. One has to say that the larger part of the manifest resources of a lab are gotten from outside grants. About half the overall budget of the university comes from federal funds. Of the other half, a substantial part is

already committed in terms of the salaries of the lab heads. We have a hard money policy -- and maintaining the fabric, the physical plant, and so forth. So the discretionary funds that could be allocated differentially to existing laboratories are certainly not more than about 20 or 25% of the total budget. All of the rest, the other half goes directly to the laboratories in the form of research grants that they apply for lab by lab, on a project basis, to the federal government, and where they have their relationship with their scientific peers, one has to say, discipline by discipline and that is a fragmenting influence.

The point I have to make is that at a major university, that ratio would be even larger, in terms of the research support from outside sources, and so there's even less of a motive for any kind of cohesive management of the overall program. Managerial challenges would come about -- if I keep referring to Stamford, it's prototypic of the major universities. I don't want to pin anything specifically on that place. I hope you don't overstate it in the final article. Stamford is typical and I know it very well; I was there for 20 years.

But at Stamford, the major managerial issue would be on the initial appointment of department heads. After

that, the dean plays no role at all. There is a quality control process, which can operate at a veto level, with respect to appointments at a more junior level, but those are done entirely within the department. And in terms of the original selection of individuals to fill a vacancy within the department, that's done entirely on a departmental basis. That's perfectly typical model. They have to be reviewed by external groups. That's the quality control aspect of it, and that's anticipated in the recruitment, in the educational process. So I'm not complaining at all about the quality of those kinds of selections. But I'm suggesting how ingrown the overall process gets to be, and little cross reference there is between departments.

One of the few occasions at Stamford that anyone in the basic sciences had of glimpsing what was going on operationally in say, the clinical department, was to sit on an ad hoc committee to review the appointment of a professor of medicine or a professor of surgery, and so forth. He had some occasion to interest himself and ask a few questions about what was going on, how that person fit. Once the appointment is made, that individual would never have another opportunity or occasion to have any responsibility in that direction. And the dean would have some role in the identification

of department chairs, but I think that was about it. I think, at any other level of appointment, he had enough to do to be sure that -- (inaudible phrase)

So in that kind of an organization, it's hierarchically structured to that degree, and there is a focus on managerial skills and management and doing are quite separated. After a while, the dean is not expected to have any great insight into the actual texture of the work going on. He's expected to be a good manager, to identify people who manifest they are capable of performing the tasks assigned to them, see to it that they have the right structure set-up, and I think after that, he's not expected -- (inaudible) very far.

And within our arrangement, we have exactly the converse, under those circumstances, because we don't have highly formalized structures. That is much more of a burden on myself and the collective group, for understanding the content of programs going on in different areas, and primarily in the group of the faculty. But here, we have a cadre of 60 potential slots, and when there's a turnover in anyone of them -- but that's a school-wide issue. Every head of lab, we have a zero base examination in the placing of anyone of those individuals. It's not even known what subject that

will be in. There are discontinuities, when a professor retires. It's by no means a foregone conclusion that that area will be -- (inaudible) That discontinuity causes problems in other areas, but it opens up opportunities for changing innovation, that really don't just exist around a structured organization. The departments are the baronies. They determine the turf that's defined for the direction of the institution, and the institution becomes exactly the sum of its parts.

Q Do you have any jurisdictional or charter concerns, even within t his 60 lab more flexible structure? Is there a possibility for two labs to both, in a sense, be seeking grants in the same area, without knowledge?

A That possibility exists. There are no formal mechanisms to override it. I would not -- I view it as my job to be sure that the lab heads are informed about what the other labs are doing. I would eventually have some concern if I felt there was an extraordinary degree of overlap, and particularly if we had to call on institutional resources. Sure, I'd have to speak up and say, you can't use -- use these dollars more efficiently, in a more eclective fashion. There's not -- I have to be a little careful about a point like that, and by and large, when I said "autonomous," I meant

it. The labs have a license to seek funding with the main limitation being what their scientific peers will allow them to have, and there is some self-regulation in that. The "Grants Review" process requires detailed exposure of the financial condition of the given laboratory. If I've already got $200,000 a year already committed, it's going to be rather difficult for me to persuade my colleagues sitting on panels in Washington, that I need another $100,000 as badly as someone who's starting from scratch needs that $100,000. Sometimes I have to try to work out strategies that would have relief "ad" limits inappropriate, and there are programs that justify more than the average funding.

But the main handle that I have there is on space. That's pretty much the resource that I have tomaintain, that I have control over, for obvious reasons. And that is understood to be sufficiently contentious, that there's been very little effort to democratize that process. I think that no one knows what procedures would be able to work if you left it open to private form of consensus, on the allocation of space.

I think one could fairly say I'm expected to know what that consensus is, and be the voice of it, without requiring a formal procedure. I'll hear about it, you know, if

the decisions I made don't make sense to a considerable number of people. I'll surely be informed about it. But we do very little vote taking here, and we have a much more informal procedure. And they expect justice and fair-handedness on my part, and I hope I fulfill that expectation.

Q It would seem that as the source of funds --

A It sounds like a family kind of organization, you're right.

Q It does sound like a family organization, yes. And a family, I'm sure, that has developed and maintains a character of a kind that probably means that when certain additions are attempted, they don't always take well.

A Sometimes that's a possibility.

Q You have a kind of transplant shock situation with certain --

A Well, I've felt very fortunate, and it's something I thought about a good deal before coming here, I felt that the doctrinal directions of the college, are both respected as fellow scientists and I felt really did dominate the political thrust of the institution, were so coincident with my own, that it would be quite a while before there'd be a serious problem, and it's true.

Q There are two different lines I'd like to follow. One is, I started to follow, is that as more and more the source of funds have come from governmental and other external agencies, it seems we've moved into a situation where we have, essentially, a demand-pull kind of research plan.

A That's right.

Q Is there any idea or crativity or push kind of research effort left, or is it almost entirely now a demand-pull situation?

A Oh, it's possible to exaggerate on both sides. I think there are tendencies on both sides. The project system of awarding funds does have very strong splintering tendencies. If the survival of a laboratory depends on successful renewal of a grant application to comply with a program that's been delineated in one thick bundle of paper in a given year, you'd have to go back at the end of three years later for renewal on that point, there are considerable disincentives to explorations in areas outside the mainline of one's original commitment.

The papers that one has published, already in the area that one's established some reputation in, continue to reinforce that reputation and you earn the funds. And I know, from my own experience -- I've had any number, as all

my colleagues have had, as well -- that trying to get out of that rut can have quite harsh consequences, in terms of funding.

I think there is a tendency to keep people in grooves that they've already cut out for themselves, and to make it more difficult for them to spread out and apply their imagination, in total (inaudible) and totally novel. And there are no devils in this circumstance. It's just built into the way these structures have evolved, with very tight competition, that results in a deliberate type of philosophy about the grants funding. And I feel it's one with discretionary resources, and they are not a large percentage of the total activity, that the main responsibility that I have in administering them, is to allow some flights of fancy, give our people some chance to do things that they're just not able to do with any confidence within the grants system.

I don't want to exaggerate it. There's still a lot of opportunity to try out some new ideas, as long as they're not a major part of one's time. If you are a very skillful investigator and can apply 60% of your energies instead of 100%, along the lines that are explicitly rewarded and reinforced, and have some left over to try out other ideas along that line, but you don't advertise it, you don't make

an issue of them until you've satisfied yourself that things are going to work, will be highly credible, and then perhaps come out with a -- I keep talking about grants, but that's the way it is, a grant application that embodies some new direction and that you already will have had enough evidence accumulated to make that a credible proposition, enough to stand on its own feet.

But the university has to provide risk capital for those ventures, so we'll have much less innovation --

The pull is also exercised, but not as much as some of the popular discussions of this would suggest, in terms of structure, admission oriented programs. The NIH is pushed in that direction by Congress, but I don't really see as much of that as is often talked about. It's undoubtedly true that there is some bias towards programs that would promise sooner applications, and do it in certain fields like cancer. And at the moment, for example, cancer is moderately better-funded than, say, mental health research is, at a fundamental level.

I don't think there are big differences in the levels of funding, and I think that's an issue, but it's one that's been ironed out over the last few years, more than is talked about. So I'm not personally as concerned about the

so-called missionary invasion of health, as many others. And I wondered, to some extent, if my colleagues aren't confusing these two phenomena. There's a little of it, but most of it is in the rhetoric that NIH uses, in terms of how it addresses Congress, that it has to put its best foot forward in terms of what the application is going to be. And perhaps with some sense that the Legislature is not fully aware of how basic one has to go, in what sort of investigation, and how long it takes to solve really hard problems. The disease of cancer is not something that can be dealt with overnight by any manner or means. And what's called targeting it, may be the least efficient way, in fact, to get into something as hard as that.

We know what the problem is. We can find solutions by targeting work in that direction. If we don't really understand the problem, one has to go deeper.

Q The difference between a NASA type situation and an NIH type of situation.

A That's right.

Q To know specifically what you're about, and NASA --

A The development of propulsion tape.

Q Yes. But you commented earlier, when you were talking about your relationships with the heads of labs, that you keep

well-informed as to the areas of their investigations, and make a point of communicating to other labs, things that would be necessary.

A Yes. You asked me how I do that. It is, obviously, large closed administrative detail. People have problems and the heads of labs would usually want to come straight to me. When they discover that I don't know as much about some of those issues, as Rod Nichols, who is my Executive Vice President, they may learn to go directly to him.

Q On administrative matters, as such?

A That don't involve policy determination. However, I think they view me as one of their colleagues, and someone who has been in exactly the same kind of circumstance that they're all in. I had an analogous role, as a professor at Stamford, and so there's a tendency to come to me first. I don't object to that too strenuously. As I say, if I do, it's usually because I'm less well able to handle certain details than other people in the President's office staff are, and we do work that out.

I try very hard to make it a rule that no conversation in this office is ever strictly administrative, and so that's one occasion that you have to chat about -- you know, what's the last thing that happened in your lab, kind

of things, and take time to do that. So those encounters are fairly numerous and it does work out just that way.

The Colloquia, another point, and I made it a point there to act exactly as I did as a graduate student, and pop up with questions. I try to make an example of not being afraid to appear ignorant or even foolish, in throwing questions -- you know, I don't understand this. And sometimes that's an excisive ignorance and sometimes it's a real one. I try to keep people guessing. (Laughter) But the main point is the posture of not knowing, is when I think it behooves every scientist tobe totally uninhibited about coming on and I do find that very useful.

I subscribe to a service that's provided by the Institute for Scientific Information, which I use in a number of ways, but particularly, I have a weekly alerting service which is profiled very specifically on Rockefeller University, so I get a notice every week on every publication that appears anywhere in the scientific literature, that had Rockefeller University on the address. And typically, there are 20 or 30 items a week, come out on those listings, and I scan those titles, and when there are topics that I'm either interested in, or they'reinherent, obvious interest, or they relate to some things I knew about before, or when I just don't

know anything about them, or surprised at what's going on out there, I'll do as the rest of the scientists do, I'll write for a reprint. But it is selective in that sense, having prescanned what is coming out week by week. And some of my colleagues will send me things spontaneously, sometimes in floods of more than I can manage. So I do try to read what they're writing, in that sense, and use an alerting service to identify that.

Then we have very frequent sessions that are part of our development effort, where we involve our faculty quite heavily, much more than most other institutions, presentations about the research that's going on in different laboratories. These are small colloquia at a lay level, but they attend almost all of those -- (Inaudible portion.) My colleagues are in those audiences as well.

There are a couple of more technically-oriented symposia. We have our annual report --

Q I've seen these from previous years.

A And this gives a fairly good precis of what each laboratory is doing, what publications, and I hope by the time the year is over, to have gone through this in some detail. And then I've made it a point, when I could, to make particular visits to laboratories. And I've never had anyone

hint anything but pleasure at my having an opportunity to come and talk to them as a group. I've done that with, perhaps, 20 of the groups so far.

And then finally, I'm trying to revive the "Welch Hall" tradition in some measure, which is probably impossible. This is alluded to over and over again by almost everyone who has referred to the history of the institution, but particularly during the time that Simon Flexner was the director, and when there were only about 25 professors, against 60 that we have right now. It was a fairly systemmatic and rather formal lunch that was used for round table discourse on specific scientific subjects, a sort of scientific luncheon club. There are a lot of obstacles to trying to do that. My colleagues tell me that the physical facility, which is now being taken over by the library -- that building was a major factor. It's hard for me to visualize that being -- anyhow, no one resists, as such.

So anyhow, I do try to arrange eight or ten of my colleagues, and say, look, I don't want to have a "no" again to lunch. No administrative matters whatever, if you don't mind. And just talk about the work that's ging on, amongst one another.

I think I'm accepted as a colleague, that I'm

able to participate in the scientific discourse, although I no longer run my own laboratory, talking about work that's going on in a variety of fields, and I wouldn't enjoy my job at all if it didn't give me both the time and necessity of continuing to touch on scientific literature. I guess this room is a manifestation of some of that. The actual content of that information is very important to me.

Q Do you think this is a critical characteristic for your success as President of the University, that you provide what is essentially research leadership, and that you demonstrate through your behavior in manifold ways, your involvement in the research substance?

A Well, I think you can add a great deal to what the institution is capable of doing. I can't really answer whether it's critical, in a sense, could it survive without it? I'm sure we could discover other forms, in the absence of that kind of role, but it makes a difference.

It's taken quite a while for me to verify it even for my own satisfaction, what the place is like, and one year's hardly enough to know that it has much impact. If I used the building of relationships to make it possible to enter into this kind of role -- one has to be very careful. I don't try to prescribe, I don't want to even appear to be in

that mode, because it would be ridiculous to ascribe to certain programs. The professor can offer his own specialty. But I can be a friendly critic and criticism is so deep in the nature of the scientific process, that done with any reasonable tact and compassion, it's the most valuable service that one can offer one's colleagues.

And I have -- I'm at a vantage point where, since I don't have the responsibility to be working with very sharp focus on the substance of specific scientific issue, instead I can and am working on, I'm on the way there, of developing a pretty broad perspective of information about what's going on in all the institutions, so I can spot where there is relevant knowledge and insight in different parts here, that I can help to bring together.

So I usually scribble notes on this respect, and -- are you aware and so and so . . . are you onto this . . . and I usually preface it, you probably already know this, but . . . kind of thing. And I must scribble four or five memos like that everyday, of one kind or another.

So I can be a kind of information central.

Well, none of those are formally necessary to maintain the organization. People have plenty of modes of communication without it. But I think it does add an extra

note to the place, and I would like it to be both effective and useful. Anyhow, it's a very large part of my (inaudible) to be here.

Q I've noticed, in our efforts to arrange to get together for this conversation, that you have often been off in Washington or were otherwise occupied. And while I was sitting waiting, I couldn't help but overhear there had been an invitation for a speaking engagement here or there. I'm sure you must be flooded with such requests.

To what extent are such things as governmental relations and external relations and fund-raising, if you will, to what extent are these concerns that vest in you?

A Well, from the institutional standpoint, fund-raising is absolutely crucial, and it's perfectly obvious that there will be -- well, no significant donor is going to want to invest in this institution without having a direct examination of the figurehead. And in fact, that follows logically from what you asked.

(End of Side 1, Tape 1.)

A They wouldn't have identified me for this role if they thought I were incapable of articulating the purposes of the institution and being able to translate those to an intelligent, but generally informed, and lay public, as far as

their own specialties are concerned, and I enjoy doing that. I've had enough experience in those kinds of relationships. And it's just very, very necessary. The flexibility, the ability to determine our own directions, to explore these kinds of options depends on private funding, and so there's been no alternative, not even a question that one can raise about whether it's a (inaudible) activity.

So I'd have to say I probably put in about a third of my time -- I don't know how you measure these things -- in fund-raising, including public relations, public interface, mostly with private donors. Our faculty deals directly with the government agency. They're only interested in pieces of paper that transmit grant applications, per se. Occasionally, I'll be able to consult with one of our professors about those documents. For the most part, they're very savvy about how to deal with the agencies, and I do nothing more than sign after they've been scrutinized by someone in our office, that they don't violate bureaucratic standards, in various ways. Those are often contractual commitments on behalf of the university, even if the funds are in fact administered by a faculty member.

So it's primarily with private donors, a bit with foundations, but depending on the scope of the program,

many foundation contacts are done directly by faculty members for specific projects, but some that have broader scope, involve more than one faculty member. I've just taken on another member of the office here; Dr. Dick Young is the other vice president; Rod Nichols is the Executive Vice President, superbly handle a great deal of the administrative detail of the organization, and by no means excluding many of the other elements I've just described.

I felt we really weren't putting enough emphasis, we didn't really have enough managerial time available, to pull together some larger programatic efforts, things that might involve clusters of five, six or seven faculty members. And one of the weaknesses of lacking a departmental structure is that there's no locus for that kind of thing. We have no pre-built organization to to "minify" the efforts, providing initiative for such groups. He's just come aboard and just getting started, but there are a number of programatic efforts that he'll be coordinating, that involve clusters like that. I'll be sharing responsibility with him, in terms of their eventual articulation.

But private donors are my principal responsibility, in terms of fund-raising. Our peculiar institution, compared to a Harvard or a Stamford, is we have, for these purposes, we

don't have any alumni. We have about 400 graduates to date of our PhD program, and they can hardly be regarded here seriously as a source of funds. First of all, they're at graduate level and they haven't gone onto the business world, sources that might be of any substantial -- in any case.

So -- nor do we have a large student body now, as a source of income, and one should be reminded that tuition fees are indexed essentially automatically, regardless of inflation, almost better than any other resource. So they are the hardest money that the university has these days, much more than its endowment. That's the circumstance.

So although, in some respect, it's a very modest requirement, we really do have to get nine or ten million dollars a year from private sources, and we have to do it primarily from moderate to large gifts from a few individuals, rather than a mass appeal. We don't have the base to go to a large group. While these are particular specifications, the skills involved in trying to define a program are rather special, and it's not the kind of thing that one can learn from a textbook. You need some common sense, compassion for the interests and concerns of other people, and learn on the job.

Well, that's enough. That takes some time,

and you are in a constant process of justifying the institution, of understanding your identity and explaining why you exist, and why it's important, what goals are represented and why they are most efficiently pursued by a philanthropic investment in this particular institution.

Q Do unrestricted corporate donations constitute a significant part?

A They are significant and, hopefully, they may become more so, because it's very hard to see, you know, as time goes on, the great fortunes are just not going to exist, and the only other obvious locus of both capability and responsibility are the corporations. So yes, that is a very significant element.

They're ones dealing, generally speaking, with professional staffs, that is, those responsible for this function are in business of identifying where these targets for philanthropic efforts are, so that presents a much easier problem of communication, basically.

Q -- can communicate a great deal of meaning --

A Publications, to a certain extent, and so on. But even there, of course, you're dealing with a wide variety of people. What happens with corporate giving is that somebody -- in the effort to minimize troubles, that some companies

(inaudible word) policies, so far as I can see, based on the notion of maximizing the number of small gifts that will keep their donees sufficiently content. They're not bothered any further. Thus, I can see the more or less political and social pressures on corporations -- it's not a terribly way to organize a philanthropic effort. And some companies do put someone other than their least qualified managers in charge of that effort.

But that's an interesting perspective to look at different corporations. Nothing will test their sense of social responsibility more than (inaudible phrase continued.)

Q How do you think the effectiveness of the university is measured by its benefactors and the public?

A Well, I think de facto, probably the only way it should be done is by the scientific reputation that the institution enjoys. You can get this in a variety of ways most effectively, by just hearing how the university is regarded by the other contacts one has. And I know the university is held in the highest esteem throughout the country, so I don't believe we have any problem in that respect, except getting that information over. There are a few objective measures, in the sense that one can point to -- you can count

Nobel Prize awards and you can count memberships in the National Academies and that sort. We feel a little uncomfortable with that sort of index, but certainly one can point to large numbers of individuals who would be as highly qualified as those who have that particular distinction. But I don't think it's a coincidence that we have very high ratios. We have about 400 graduates, and I still find it almost unbelievable, two of them have already won Nobel Prizes. That's speaking for something.

Q What is that?

A Probably the appeal of the university for very, very qualified individuals. So we're transferring the locus of measurement of esteem, in effect, the kind of student who is going to win a Nobel Prize will decide to come here, and doubtless have very good reasons for it. At least he's not obstructed in his own development by the kind of institution we have.

I find it hard to say that out of a randomized set of entries, that the stamp of our education is what resulted in their getting that distinction. I think it's very good, but I think they have to have been very well qualified, in the first place.

Q Yes.

Q Do you think the staff would evaluate the effectiveness in the same way? You know, the people who are the institution. You know, there are those who are outside and look, and have a certain criteria --

A If by "staff," you mean the personal staff --

Q I mean the 60 labs.

A I think they can form their own judgments by their immediate contact with the other people here, and by publication. Publication is the name of the game. I think, both, at firsthand, by general reading of the materials, and how they hear their peers talk about them. So it isn't all that different, except there's the additional ingredient of the first order of evaluation.

I think there are certain measures of quality that transcend the technicality, and when our people talk about their work to our -- we have evenings to which we invite prospective donors, people who are interested in the institution and they can meet them face to face. I think something comes through with that interface as well.

Q When you are involved in going out and looking for someone to join the staff -- I don't know how often you're required to look at the head of lab level, or at subordinate levels --

A Twice a year, at this point. It's a pretty stable institution right now.

Q What are the major criteria that you use in seeking out individuals to join the organization?

A Well, I think an aggressive intellectual creativity, with the obvious skills within the particular area of interest, but I wanted to put more stress than we've ever voiced before on how they then augment the community. I think, hypothetically, it's even possible that there might be even the most brilliant individual in the world, who would add little to the community "gess," and I just feel the latter has to be a consideration as well. Now, that's reflected, in large measure, in a particular field of interest that person is working in, and does he or she bring a new dimension of scientific capability, in terms of the specialized field. There are also personality factors. We're talking about people who enjoy relating to others in this unique setting, where there is so much opportunity for disciplinary discourse. So I feel that's one of the very strong assets of this institution, in regard to preference with respect to people, both to reinforce the tradition, but also to take advantage of it. They work together.

Q So it is the intellectual vigor and resources of the total community that contribute, in substantial part, to

each of the elements of the community.

A I think so. Now, you don't create that ensemble unless the units themselves have extraordinary capability, as well. They have to work together.

Q Has research, in a setting such as the university, to a great extent -- or productive research, to a great extent, still primarily the function of the outstanding individual, or is it primarily the team that is put together in a given lab?

A Well, it's impossible, with the rare exceptions, to research these days without a fair amount of technical help.

Q Yes.

A So there is, I think, almost no laboratory where that doesn't play a significant role. I can think of one or two professors who have skills, literally, with their own hands, that they don't share, but that's the exception rather than the rule. The head of lab designs the strategies, designs on the problems, develops the techniques and guides the work. He may only occasionally actually handle research materials with his own hands. But the groups may be anything from a total of two people to a total of forty. They vary a good deal, as to the kind of field that one's in. Some areas of

research require very complex coordination of different agents and instruments, and very specialized techniques, and others could be done by an individual person.

We still don't have much of what we would call big science, in the sense of large, complicated teams where there is usually multi-faceted direction, which you have particularly in physics, if you're working on a space program. If you're working on a million dollar instrument, you don't do that yourself, at any level.

The labs are such that I would say, with very few exceptions, the professor at least occasionally does actually handle the materials, and who would, under any condition, would be quite capable of doing so and will. If something is going wrong with a technique, he'll come into the lab and work it through his own hands until it's set right, and will then delegate that to a technician, or show a student how to do it, and the work will then be done, very largely, at that level. So it's a mix of immediate and not quite so immediate involvement. I think that's quite typical of research at universities, and it's no different at Stamford than here.

Q Within the community, do people move from group to group?

A Not very much. Students do. We have graduate students -- are admitted. And we have 20, 25 of them a year. We have a small admissions office that guides that process. And that's done on an all-university basis. These are not selected by individual laboratories. So when they come here, they have to start making up their mind almost immediately, what areas they'd like to specialize in, and students quite often will involve themselves with several laboratories, before they finally settle in with, or do their dissertations on. Typically, they'll take five or six years in the process.

Junior faculty -- well, the next grade would be our Post-Doctoral fellows, and that's an informal level of training, in the sense we don't have a certificate or a named degree, but it undoubtedly is our most important educational output. These people with a PhD already, are here for a couple or three years. It's quite specialized advanced training, actually doing research in a given laboratory. They come with a considerable set of skills and they bring them to their final pitch during that time.

These people would be recruited directly by a lab and they're generally funded directly out of the grants funds, that that lab has, although we do have some training grants that have some (inaudible word) range. And there, it's

only exceptional that they go from one to another. And then we have our assistant professors, a still more senior grade of post-doctoral fellow. These are people we expect to be here for some years, although we could never offer them the likelihood of tenure. They may be here for two, four, six, eight, ten years and have reasonably stable positions during that interval.

And then, again, they might move around if lab head were to retire, or if there would be some divergence of research interest. Once in a long while, you get a personality clash and so on. But the assistant professor is expected to be here for a while, so the possibility of finding another lab to work in is a little greater. They'll be looking out to see all the alternatives during that period of time.

We have a policy here of requiring very careful review after no more than six years, of the prospects of an assistant professor. Their appointments are actually reviewed by -- that's essentially done in this office. That is, a lab tech will make a recommendation, either for an initial appointment or for renewal, and will provide me with appropriate documentation about why that person meets the quality standards and they'll see about funding, and so forth. Those are almost all funded out grant funds. There are a few positions exceptional to that.

By the sixth year, that individual should either be a candidate for promotion to associate professor, which is a little further notch of stability in salary and prestige, or had been advised to leave, and would have been advised sooner than that, if that were the likely outcome. There can be all kinds of reasons for it. In many, many instances, it is simply that they had reached a maturity, that they should be in a more independent position than they have here.

There is a little more hierarchal structure here than in universities, within the laboratories. As an assistant professor, even an associate professor, may in fact be reporting to a lab head and be part of a somewhat organized research program, to a degree that would not be the case in a university department; at that level, you expect it to be more autonomous than is the case here. And that has its pros and cons. It does have some influence on the texture of research. However, these are not permanent appointments, and after a maximum period of 12 years, we do, in fact, have an "up-route" on principle that they either qualify for tenure, which is very jealously guarded item on the hard money doctrine, I mentioned before, or they've gone elsewhere.

As I say, the issue is almost always resolved by the fact that they reach maturity and independence by the

fourth year, sometimes the sixth year. The junior faculty will have already been applying for research grants in their own name, and once they've got into that system and have been successful at it, then they have a high degree of mobility. It's a much more comfortable situation, I think, about moving to another institution, once you've qualified for research grants that you can take along with you.

Q You know, in a sense, it's very entrepreneurial. They are small -- forgive the expression, perhaps, but they are small business persons who have an entrepreneurial concept. They go to the market, they are successful, and thereafter they can be quite independent in the locus in which they'll operate.

A That's right. That's one of the things that they can learn in this kind of framework, they learn how to work that system.

Q Yes.

A By very close involvement in this operation and they're part of successful -- Tenure implies being the head of a lab, and it is now essentially a lifetime commitment to the late 70's, and with an extraordinary degree of freedom to do just what one would really like, with one's career. So it's a step we don't take lightly.

Q You mentioned, earlier, when we were talking, that in terms of selection of staff for the laboratory, that it is the lab head who is the key individual in making those selections, but that others participate in reviewing the credentials.

A Well, there are different stages and there have been some changes since my coming here. As it is now, assistant professors are nominated by a lab head and I do, in fact, make a review of the nominations. That is, the credentials are documented to make a credible appointment, and I'll call in whatever pertinent advice I feel is necessary to justify it. I'm trying to -- the phrase "assistant professor," had different connotations years ago than it does today. We started as the Institute. At that time, people were called members, associate members and assistants, and the assistant, at that stage, was really more like a post-doctoral fellow than assistant professor. When the titles were changed, there was still some ambiguity about that level in the ranks. But since we advertise to the rest of the academic world about the quality and standing and maturity of our personnel, using their phraseology, I feel that we really have to put up people who pass muster by those filters. And so there's been a gradual change on that score.

And I'll call for some external documentation

at that level.

At the rank of associate professor and above, there is a formal faculty procedure, similar to the ad hoc committees that I described elsewhere for reviewing those appointments. However, the initiative still comes from within the lab and it's only in the appointment of a full professor that we have a collective university process for identifying the area. We have a group called the Academic Council which is an elected group of representatives --

Q Heads of labs?

A Yes, they're heads of labs.

Q Only heads of labs?

A That's correct. So we have six or eight of these members of the Academic Council. They rotate onto this committee, and they're the guiding body with respect to academic qualifications. So these matters of credentials are established by them. So when there's a nomination for an associate professor or the establishment of a Search Committee, there's an aggressive, no holds barred search, for the full professorships. The associate professorships are nominated by a process that starts more in the laboratory. I work hard to make sure that there's really been -- well, the criteria given, and I ask, provide whatever evidence you believe is most persuasive

on this point. I want to know that the person coming up to that rank is of the quality of the best person that you could hope to recruit here, on a nationwide basis. Now, you can define the job, you can the disciplinary area and so on, and that question can be answered in a variety of ways.

In some institutions, a very formal search is done that literally interrogates everybody around the country to answer that question, and ends up being a very cumbersome process which, in most cases, is just a waste of time and a lot of energy. There are some areas in which some fields of inquiry, where anyone who has read the literature, will know who the leading individuals are in this respect and pursue the search, accordingly. Others might have to search under a barrel to find appropriate people.

So I think an inquiry, circumstantial as possible, just to reduce delays and unnecessary paper handling -- but I do require persuasive presentation.

Q I'm not quite certain how the faculty at the university is rewarded, and what kind of incentive program, other than the prestige and the universal acknowledgement by their peers.

A Well, we do have a salary review on an annual basis,

as well. That's done in this office. There is a component of it that's market-driven. We feel there are standards of compensation that have to match what I know is going on in the rest of the country.

Q At prestigious universities?

A That's correct. Market-based. And then some small margin within that is intended to be connected with the issue of the market, for people who one believes are performing their jobs with particular skill and insight and success, and who are also the ones that other institutions would be likely to make a bid for. So some small percentage of discretion is used in distribution of salary levels. This is a matter that's left to the discretion of this office and the Board of Trustees. We don't have any public proceedings and it's regarded as highly confidential, just what salaries people are being offered.

But I do ask myself a question, as to what's likely to happen if Harvard or Stamford makes a bid for so and so, and are we going to be in a position where we're so far behind in our financial rewards, that he can't afford not to respond. We try to anticipate that.

Q You mentioned the Board of Trustees --

A But I have to say, in that connection, by the way,

because this could be a very misleading picture -- the fact is that, year after year, this is a national phenomenon, that salary increases have been considerably less than the increase in cost of living, and the relative economic position of individuals, over years, I would say, as I say this is a national picture, but I think we're down 10 or 15% in income, from what these same individuals were getting eight, nine, ten years ago. And my understanding of the national statistics is that the labor force, in general, is about at par -- gives a little bit -- but it has managed to just barely keep up with changes in the cost-price index.

As I say, that's not special at this institution. But I think we're roughly at a par with the institutions that we relate to.

Now, there's a cadre of people who are exceptions to this, and these are the clinical sub-specialties, and their remuneration has -- is totally different from what goes on in the rest of the academic world, through market pressure. So we don't even begin to compete with the incentives that would be possible for people who actually went out into medical practice. I guess the idea is he has to decide what kind of career you want to make for yourself, and if you want to be involved in the intellectual excitement of discovery, and some

elements of stability, reassurance and you just have to make your choices. (Inaudible sentence.)

Q You mentioned your working with the Board of Trustees on certain matters in the conversation. What is the nature of your involvement with the Board of Trustees, the Executive Board?

A Well, it's fairly intermittent. The Board, as a whole, meets only three times a year. The Executive Committee has occasional meetings at other intervals, but only rarely, if some special problem comes up. Pat Haggerty is our Chairman, and I have considerably more frequent conversations with him, and he will often telephone other members of the Executive Committee, particularly in terms of intermittent policy setting. So we have a strongly involved and experienced chairman, and he makes no effort to involve himself in academic policy, but he takes on a very stringent responsibility with respect to the physical affairs of the institution, which is entirely appropriate. I feel that I'd be at a disadvantage, given the realities of the circumstance, and that's what drives it, because we are still in a deficit situation, if there weren't both thorough appreciation of this and the fact that the Board, as represented by the Chairman, takes responsibility for it. I mean, there are hardships and consequences of those

realities, and if there were any confusion about that, I'd be in the middle. That just wouldn't work.

So there's no divergence of opinion about meeting that bottom line. The present circumstance is that -- and I mentioned this to you when I was talking bout Fred Seitz' problems over the years -- for a period of about ten years, there was a very distinct deficit, no matter how you measured it. There was some retrenchment. The work force was reduced primarily by attrition. The (inaudible) program was let go and there were some other areas that were held back. There's a lot of maintenance around here -- there's lots of science -- we have to keep up with that. And a very strong effort, a development program, pulling in more funds from federal sources, as well, so we're almost in balance right now, a combination of those efforts.

The present picture is that -- well, one way of describing it is to say we're okay in our operating budget, but our capital budget puts us below the line by about a half a million a year. That's probably understating the problem, because it doesn't take account of the erosion of the real value of the endowment. There may be no pat-out policy that will enable us to keep up with the 13% inflation rate. What could you do? But even if you regard that in the clinch,

we've been paying out around, just under 6%. And I think for several years, that's been more of the pay-out rate than the real earnings will allow an endowment. So there's been a gradual erosion there. And we'll just have to meet that by increasing all the other efforts. It's hard for me to see that we can do very much more by way of cost control. We have some essential operating expenses here, as far as they will go. We can't push our work force harder. They're already in enough trouble, and we just can't do it. I mean, they're hardly keeping up with inflation, with the national standard, in that respect.

So we just have to raise more money. That's part of what I said earlier. We're about a million and a half a year short of what are our appropriate goals, for balance, not for growth, but just to be able to stay viable. I think we'll get there, but we'd better get there a little sooner than later.

Q Looking at the research scene, from an overall national basis, do you think that the various institutional elements that are contributing to our national research posture, given the more or less ad hoc procedures that we have for bringing about coordination and making use of the comparative advantages of one facility over another, and given there is research, do

you think, on an overall basis, the research effort is reasonably well coordinated -- I hate to use the word "plan," although there are certain central agencies that engage --

A Well, my view is that to do more of this in Washington would make things worse, but that institutions should take more responsibility for their own local coordination. I've obviously been exemplifying it, in what I was saying about the Rockefeller. I don't think it's going to work very well, in terms of national planning. I don't think anyone knows enough to be able to tie it together more aggressively. The game word is that of discovery. We're trying to find out what we didn't know and didn't understand and could not predict was important. The discovery of an important question is far more important than we get the answers to what we think the questions are. So I think, if anything, centralized planning -- I'll say the rhetoric of it has gone far too far. The substance of it is not as much as the rhetoric, and it's probably gone further than it should. But that's -- you asked about Washington. My main involvement with Washington is not in fund raising. As I said, my faculty goes through the (inaudible). But I have a number of places in which I can try and participate in thinking through exactly these kinds of issues, science and research policy. A considerable amount

of the time I spend is in that connection. That's true with medicine, the National Institute of Health, and so on. I think there's a lot we need to learn about this process. It's not one that's well understood or that well investigated. I mean, how many people have the right to say that they have some expertise on these matters? I have some observations about it, having been a front line soldier; from that perspective, I have a responsibility of generalship, and the people that do have the same kind of experience and background that I had before, I think, can be trained to ask some of the pertinent questions. But I think there's a great deal to make further inquiry about.

The point I feel really very strongly convinced about is that when discovery is the essential ingredient, that you want to maximize the conditions under which discovery can be elicited, you can't possibly tell people how to make them, and I think that's the essential lesson. And I think some of the things we do now, quite by inadvertence -- I mentioned before about the specification of the research project, the dangers that are imposed and perceived by deviating from stated goals.

I had an experience, I guess it was about two years ago, now, the last set of research grants I was working

on at Stamford, and I discovered I was going to move. I had filed for renewal on a program that I was working on for some 15 years, and I expected every time to have very substantial scrutiny. My usual strategy had been to put in a very detailed research report. Look, this is what I've done. This is what you got for your money the last two years around. I intend to work in the following areas. I have these kinds of directions and these kinds of questions that I want to pursue. You can read my bibliography to find out what you need to know about my skills, in pursuing them, and here are five or six other literature citations that will describe what the current state of the art is in those areas. You know them and I know them and there's no point in my copying them down in a detailed document.

So it was a really detailed report on past performance and a very sketchy outline about what I was going to do next, and a remark that I really wasn't that sure. But if I were lucky, I'd have to throw all the plans out the window, because something so shining would come along that I couldn't afford not to pursue it.

And for the first time, I was really slapped back hard, the procecure. I was told that this is unacceptable, in the present climate. You have not defined your research

project. We hold you in great esteem as an accomplished investigator, but that this is no longer the criterion that we can use, for the approval of research grants. And the usual way that that game has been played is not an outright rejection, but an approval but at such a low priority rating, that it was impossible for it to be funded.

Well, I knew enough about the system to know that that was really fatal, so I pleaded and did get, instead, a deferral. They would withhold judgment on that question, and if I had some other information to put in -- I had quite a few agonizing nights about the whole system. All the hypothetical issues that I've been describing to you were dumped right on my head. My career was at stake, and much more than people outside could believe, I really needed that particular grant if I was going to stay in the laboratory.

It was a complication that might have confused the issue. I had administrative responsibility for quite a large amount of funds that had to do with departmental operations, in which I was not involved. I was proxy for a number of other people. Well, I was there in the picture and I suspect a number of the committee felt, well, you got 1.2 million dollars after you name; do you really need the

$60,000 for your research grant. Well, the fact is I did, and the fact is, they never asked me whether that was, in fact, the case, or to justify it. I'm sure there was some latent assumption that that could be worked out automatically.

I responded by making an analysis of all the previous applications that I had submitted, a half a dozen of them, previous history, and I summarized what I put down as my research project, and also what were the significant publications, and as I suspected, and actually worked out to be the case, but to a startling degree, there was not a single instance of a significant publication that had been anticipated in the application. You know, there were a couple of dozen of what I'd have to say were significant in importance, out of that entire group. In fact, the Foundation of a considerable industry, the people who work on them, carried them further in the whole field of bacterial genetics, in a wide variety of ways. But not one of them had been anticipated in the application. And I said, what kind of rigamarole do you want me to get into, to try to define a project that I know I'm not going to work on? This is my style of discovery. It's an exploratory style, not one that's scratching in a groove that has been through there before.

Well, with a lot of heartache, it was eventually

persuasive. The fact is, I faced the termination of a laboratory, and that was an institution that didn't have a flexible funding back-up that we have at Rockefeller.

I'm afraid there are several lessons -- I'm afraid that's becoming almost a daily event in our game. There's a random process, by the way, of whether or not a line of work is going to continue to be funded next year, as compared to last year. There's just so much pressure with essentially flat-funding real dollars in the last ten or twelve years, and an enormous growth of opportunity. There are so many new technologies that are on the line. We have a whole new generation of scientists that we've trained, and they are trained now to be our competitors, so there are a number of people that are competing. The areas of investigation that are pertinent to investigation have broadened very substantially. So the competition for those limited dollars is very intense.

So we're now operating in a framework where about 20% approved applications are in fact funded. I mean that's what, obviously, is connected to the shift of emphasis in a highly bureaucratic mechanism. If you have to say no to four people out of five, you have to have pieces of paper that you use to defend those kinds of judgments. They aren't

villains in the system, but it's almost a structural tragedy.

However, there's one element of --

(End of Side 2, Tape 1.)

A (Continuing) You try to find and identify creative individuals. We have to be very discriminating in who you find, who can offer real evidence that their ability to perform and to do this kind of work -- that can be either the scientific project that they outline, the work they're going to do, or their own recent accomplishment. But by whatever means, the focus is on the excellence of the individual. And that has been explicitly rejected, in terms of a formal policy, in language that I just quoted to you. It's straight out of the book, in that respect. The project has to be, in some way, a criterion of the award.

Well, I think that the business of our institutions, a place like the university -- we're in the business of locating people and to try to identify who they ought to be, and then try to give them the opportunity to continue work at their highest potential. It isn't just a matter of providing shelter and comfort. Sometimes that means prodding and asking questions and providing the critical framework and demanding certain kinds of performance. But the answer is still is

(inaudible phrase) and it's my responsibility.

Q If five years from now -- let's take just five -- five years from now, if we were to sit here talking as we're doing this afternoon, what kinds of things would you like to say, what kinds of change, perhaps -- well, basically the kinds of things would you like to be able to say about the Rockefeller University, that you would attribute to your being here and to the kinds of efforts that you anticipate you will be making.

A Well, I'd have to say, first of all, that we're still able to exist proudly, with the traditions that we've had before. That's no mean task. In fact, I'd have to say that tradition is such a strong one, such a productive one, that if that were all that could be said, it would still be very satisfactory. I don't have aspirations for enormous change. We have a very special kind of place that is under a lot of pressure and it's a very responsible stewardship to keep it going.

I think, to do that, will require all the measures that I talked about before, definition of self-identity and so forth.

There are some subtle changes in style and texture that I would look forward to, a very few programatic

directions that I think (inaudible) getting into.

For example, with all the strength that we have here in neurological research, both at a basic and clinical level, cell biology and even substantial behavioral sciences group, we have no work at all on psychiatric disease. And schizephrenia and depression are such important public health problems and the kinds of perspectives that people already here will be able to offer to a program, we have enormous leverage to establish work in that area, and I'm working to try to do that.

Another programatic direction that is a little less obvious and has a larger policy and social utility impact is an area that I call comparative toxicology. But I have to state that that's a scientific basis of risk assessment of toxic hazards, from our environmental sources. Now, that may seem to be a rather specialized subject. In fact, I think it's our most serious public health challenge today, at many different levels. First of all, our economy is now hostage to the accuracy of our perceptions about public risk, whether we're talking about nuclear power, the chemical industry, the pharmaceutical industry, but more and more, wherever you turn around, you find the issue of the question of public liability, comes up. That has been thought of as being some sort of side effect

that we can hope to clean up after the fact and I think that's a totally wrong conception. It's a question of the safety and adequacy of our procedures, with a central issue that has to be part of the initial design of technological innovation. I mean, with medicine and industry. If you stop and reflect on it, you'll find that there is hardly -- there is no technology that is proposed or under active consideration, or actively deployed, with the element of unexpected risks dominating our consideration. Think about the kill, think about the (Inaudible) 2 episode, built into the whole fabric of drug development, and 95% of the costs of drug development are the satisfied risk contributions; nuclear power or any other form of energy you might go into, they are no longer side issues. It's a central one to our environment and to the relationship between science, scientific judgment and policy formation. And we don't have very good mechanisms at any stage to deal with it.

Now, this university does not have strength in the established disciplines of economics and sociology and political science. It might deal with the policy end of it. But even the scientific evaluation of toxic risk is in a horrible mess right now. And that, I feel, we do

have an urgent responsibility to deal with. And it's also exciting biology, because now we approach the question of whether a chemical is risky in mechanistic terms. You don't want to just answer a question, how many mice are killed if I inject such and such a dose. We're in a position to ask questions. What specific interactions with the cells or the chemical constituents of the body or the nervous system or other organ systems, are responsible for that toxicity, and in that kind of a framework, we have a certain predictive power that doesn't otherwise exist and we need that intervention. First of all, we don't want to do experiments on people, to assess risk. We want to do experiments on laboratory situations. Well, that implies a kind of theory. We have to have some theoretical framework upon which we can confidently extrapolate what we found by experiments with 500 mice, at such and such a dose, as to what is going to happen, possibly, with public exposure. It's just not done at the present time.

Well, that's, I think, the main issue, in terms of predictive capability. We've got to do it as between different dose levels, different elements -- we should also be in a position to predict, to design our chemicals, and say, from what we know now of mechanisms of toxicity, can we see our way clearly to avoiding problematical situations

in the first place. That's something that could already be done to a much larger degree than it does now, but there's a very long way to go. To get the theoretical basis on which to structure a molecule, I'd say, I'm pretty sure that's going to be toxic at such and such a dimension, or we'd better test that first before --

To give you a very particular illustration, we rely very heavily on antibiotics in dealing with bacterial infections; as you know, from recent gonorrhea epidemics, for example, the bacteria eventually adapt and become resistant, and so it's a constant race. The fact is there are tens of thousands of substances that we know will kill bacteria. Most of them have been tested only very superficially and it's almost a blind man's buff, right now, how the sequence kicks out from it already has on the shelf, which bacteria killing agents I think we'll invest in further, to try to see whether they can be developed for antibiotics, in a practical sense. And the central issue is almost always toxicity side effects. The fact is, we don't know. We have almost no rational way of how to predict which ones of these would be worth further investment, and some people have somewhat more of an act than others, but I think it's pretty much a matter of chance which ones have made it. At the present time, you have

to invest several hundred thousand dollars even to do the preliminary kinds of tests, to determine whether it's worth while carrying on further.

Well, given what's on the shelf, we could have a hundred new, effective and hopefully safe antibiotics if we knew how toget past that first hurdle. In other words if we had a better theory for estimating toxicity, and that's been just a very narrow domain.

Well, anyhow, I'd like to see us -- we again have very high leverage in terms of existing capabilities -- it's a wrinkle on how we use biological science for health that's not adequately developed anywhere. The main emphasis in therapy is what you're going to learn that's going to help you treat a disease a little less often. It should be much more what you learn that will prevent disease. But risk estimation, I think, has to be elevated from being a side issue to being a central one, if we're to get anywhere.

Well, those are the main programatic areas that I'd like to see developed. As both of them illustrate, the new initiatives would have a pretty deeply inter-disciplinary base, not just things going off on their own, where utilizing the strengths that would involve a particular star investigator, and getting that laboratory going. And that quality of cross-

fertilization among the different activities that we have here, we have a basic science group, we have a clinical group, we have a field station doing hospital work, and while I have to say that the intra-communication, that a "raids" list surpasses anything that I know anywhere in this country, I think we can carry on still further and more fruitfully and make this a more exciting place to work. That's what I mean by building an academic community. Well, that would be my other criterion of change. It's not a drastic one.

Q Is there anything else that you think should properly be in such a record as we made this afternoon, that you would want to define?

A Well, I've only talked in the vaguest terms about research management, although I think that comes through very clearly, from how I described centralization from Washington, what the job of management is. However, other organizations do require more of a discipline of low orientation than we've build in here. We're quite content here to make contribution in almost any arena that's relevant to health. Whereas, in an industrial context, there may be narrower goals. I think it's the job of management to define those goals and to be very careful about

centralization their implementation. If you can convey to the people who are actually confronting nature in the laboratory and get them to internalize those efforts, that's about as far as you ought to go. They'll be far more capable when using their own imagination and direction, and the observations that they make from day to day, to meet those goals than any central manager at any level. So there is that responsibility.

Now, the converse of that, and I don't think that's one that's always sufficiently accepted as a responsibility, I think academically oriented basic investigator ought to be left alone, in terms of their decisions about what research they pursue. I think they have a responsibility to inform themselves about what the needs of their immediate community are, the social needs with respect to health are, the basic bio-chemist ought to be knowing something about what happens in the clinic, in terms of what the requirements are, for this information. And I think if he's informed about that, you're not going to have too much trouble about further developing an interface. And you have to build up incentive structures to make sure that assumption is, in fact, substantiated.

Q Very good, fascinating.

(End of Side 1, Tape 2.)